would seem to be indisputable, then, that the American people are satisfied with their buildings if the outsides are good-looking. The structures illustrated in the *Record and Guide* include private residences, apartment-houses, hotels, warehouses, and churches, any one of which must have required some ingenuity in arrangement of plan, and have had some interesting constructive details, but they are carefully hidden from those who should be interested in these essential portions of architecture.

These indications of the tendency of American architecture show very clearly where the error is. The needs of the public are heeded in almost every phase of modern life and thought. The manufacturer and the shop-keeper, not less than the editor and the artist, are continually on the lookout for what the public wants, and hasten to supply them as soon as manifested. The public evidently want only exteriors in architecture. Plans, use, environment, and other matters which were once pre-eminent in the art, are now at a discount. Until the popular mind frees itself from such erroneous ideas, it will be impossible for the art to make any progress. It is well to remember that the general public which is satisfied with such things is more to blame for their continuance than the architects who prepare the designs; but it is a serious retrogression when the architects join the popular movement, and give their assent and support to it by catering to its most objectionable features.

BARR FERREE.

School of Architecture, University of Pennsylvania, Jan. 8.

Cyclones and Anticyclones.

IT seems to me that the discussion in regard to the origin of cyclones and anticyclones that has been in progress in *Science* and other journals for several months past opens up a question that has so long been regarded as settled, that it seems impossible to look upon it as being in doubt. It is, in short, as to whether gravitation is the chief cause of movements of the air. Barometric observations have directed attention so forcibly to the relative weights of columns of air in storm-centres and elsewhere, that it has been assumed as a matter of course that the pressure gradients thus made manifest are the occasion of the horizontal movement apparent as wind. If this be the true explanation, in order that such horizontal movement may continue, it is necessary that there be a corresponding vertical movement, and that it be sustained by adequate renewal of the buoyancy of the air in the proper localities. This renewal of buoyancy can only be accomplished, so far as our knowledge at present extends, by heating. But now we are informed as a matter of fact that the air at anticyclonic centres descends in spite of its being warmer at an elevation, and in like manner above cyclonic centres fails to descend, although colder than at the surface of the earth. This certainly opens up the entire question as to whether there is ascensional movement at storm-centres commensurate with the extent and velocity of the winds blowing horizontally, and supposed to be due to an indraught; or, in other words, whether gravitation really plays the part that has been tacitly assigned to it, or whether it must be relegated to a subordinate position. Personally I am very glad indeed that a discussion having such bearings has come up at this particular juncture, because it has increased very decidedly my interest in following certain clews that look promising in regard to the effects of variations of the earth's magnetic condition as a whole.

M. A. VEEDER.

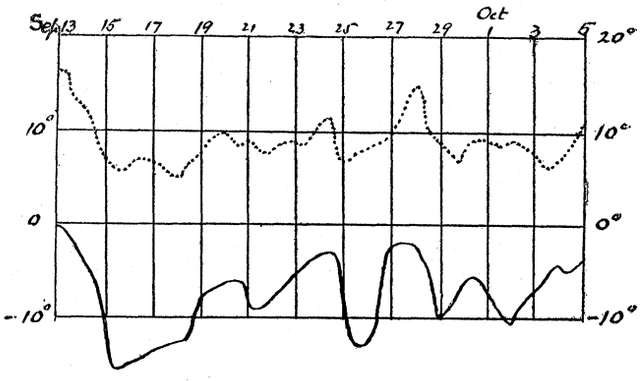
Lyons, N.Y., Jan. 5.

Dr. Hann and the Condensation Theory of Storms.

THE time has not yet come for a review of the various discussions upon this subject that have been published during the past four years. I doubt if there has ever been a better illustration, in the history of meteorology, of the absolute necessity there exists of appealing to observations in order to establish intricate theories, than the recent discussions on the reversal of temperature in our storms and "highs," which is but another way of putting the problem before us. In this very line Professor Davis says (*Science*, Jan. 2), "Records of temperature made on high mountain-peaks furnish the best means of testing the convectional theory of cyclones; for, even if all other tests were successfully borne, failure under this test would be fatal to the theory." This statement of the case should be received with a little caution, however, because the presence of the mountain must be a modifying cause, and oftentimes there are cases in which some part of the storm, or high, has its action below the mountain-peaks (I have found this true especially at Pike's Peak); but the larger commotions of the atmosphere may be profitably studied at such points.

In carrying out my studies on this problem, I have invariably sought for help from the original records, which are now so abundant at Mount Washington, Pike's Peak, and at many high stations in Europe, and I have massed thousands of observations bearing on the question. The first publication of these studies was in the *American Meteorological Journal* of August, 1886, in which I showed that the temperature observations at the base and summit of Pic du Midi, in France, indicated a decided rise at both points on the approach of a storm. In October of the following year I showed by the observations at Mount Washington that in both storms and highs there was the same fluctuation at the summit as on the base, and that the mean temperature of the air-column was ten to twelve degrees higher in storms, and the same amount lower in highs, than before or after the centre had passed.

It seems to me that the crucial test in Dr. Hann's recent work, which has attracted so much attention, must be the records at the mountain stations, and I believe that this will be insisted on by Dr. Hann himself as strongly as by any one. In fact, Dr. Hann has based all his work on his interpretation of the records.



TEMPERATURE FLUCTUATIONS, 1889.

Sonnblick, full curve; Salzburg, dotted curve.

It seems to me that he has given altogether too much weight to a few isolated cases, while he has ignored hundreds of cases which disprove his propositions. I have already shown in this journal for Sept. 5, 1890, that the evidence at Sonnblick is different only in degree from that in this country, and I have there explained how the peculiar results in the remarkable high of barometer, 1889 (which, in fact, was the only one in three years exhibiting such discordances from the usual law), might be accounted for. I have now made a special study of the storm of Oct. 1, 1889, which Dr. Hann advanced as favoring his view, that the temperature in a storm falls as we rise in its centre, and at some height is lower than that of the surrounding region. The results of this investigation so remarkably corroborate my position, that I present a copy of the curves in order that others may see the exact state of the case.

These curves are constructed as follows. The lower or full curve represents the temperature observation for each day at Sonnblick, 3,095 metres (10,154 feet), at 9 P.M., at which time very nearly the mean for the twenty-four hours occurs; and the upper or dotted curve shows the temperature at precisely the same time at Salzburg, just north of Sonnblick, at a height of 437 metres (1,434 feet). I have given the curves from Sept. 13 to Oct. 5, including the storm of the 1st. It will be seen that there is a most remarkable accordance between these curves; almost every bending at the base is faithfully reproduced at the summit; and, if any thing, there is generally a greater fluctuation on the mountain than on the plain. This is not all, however. Examining the very date under discussion, Oct. 1, we find that at Sonnblick the temperature began rising on Sept. 29, and in twenty-four hours had risen